



THE ROCKEFELLER UNIVERSITY

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PRESIDENT

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Dear Tom:

Thank you so much for sending me your books. I am particularly looking forward to reading your biography of Robert Koch. I cannot say how much I admire the energy that enables you to bring these projects to fruition so quickly; and I am looking forward to your work on bacterial genetics with even keener interest.

You will not be surprised if I send you some material from time to time bearing on that subject. I am sure you are already very familiar with the review material that appears in some of my own early papers circa 1948-1953.

I am also glad to send you a little tidbit on the history of the discovery of lysogenic conversion that has puzzled me until just now. It took a little detective work to locate Victor Freeman and I was surprised to discover that he had been a near neighbor during much of the time that I was at Stanford!

With respect to the history of bacterial genetics there are a few points that I hope you go into more deeply than the usually rather superficial accounts that have appeared to date, items like:

- ° The reception of Avery's work
- ° Exactly what did Luria and Delbruck 1943 prove!
- ° The jumble of confusing hypotheses that Hayes and Wollman threw up before the wonderfully elegant experiment of Jacob and Wollman on progressive entry of the bacterial chromosome.

With regard to the Hayes episodes I think you might find it very worthwhile to have a conversation with Luca Cavalli-Sforza who can be found at the Department of Genetics at Stanford.

* Cairns' Nature came out just after I drafted this letter.

Dr. Thomas Brock
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May I also especially commend to you some of the earlier harbingers of real insight, one of them right at home in Madison: a paper by Wright and Cole -- keep in mind how soon this followed on Johanson's publications.

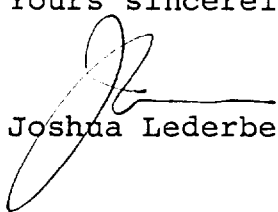
I gave a talk at the Pasteur (from some rather jumbled notes but which I hope to collect in a paper) about antecedent insights which have eventually paid off, as well as the blanket of confusion on genetic perspectives on bacteriology that suffocated the field roughly from 1910 to 1940. There was a wonderful story going back to Pasteur himself that you are probably familiar with, namely the attenuation of anthrax by heating the cultures: that turns out to be the loss of a toxin-coding plasmid!

With respect to postmature discovery, Harriet and I are now leaning to a formulation that embraces resistance to discovery once published, and deterrence of discovery that, operating at an earlier stage in the development of a scientific idea, prevents or delays its actual fruition. We'll be sending you more about that in due course. There has been a lot of argument about "post-maturity" but no one argues about resistance; and if discovery can be resisted it certainly can be deterred.

I will be writing some more short chapters on specific episodes in my own work, including replica plating and indirect selection, the plasmid concept, and if I can get Norton Zinder to agree to renew our collaboration, one on our discovery of transduction in salmonella.

I am sure you will not overlook Jim Crow as a resource: he joined the Genetic Department in 1948 and was a pretty close observer of what was going on there. He also knows the history of genetics very well indeed!

Yours sincerely,



Joshua Lederberg